Response to Editor's comments

We thank the Editor Dr. Qiuzhen Yin for the time invested in reading the manuscript and the reviews in such a careful and thorough manner. The comments have been carefully considered and responded. Please find below our response to each comment.

General comments

1. ***As you will see, Reviewer 2 still has some concerns and comments. The major concern of Reviewer 2 is on the uncertainty of using the single model simulation. In the meantime, the reviewer also acknowledges the potential cost/difficulty to solve the methodology issues raised. As a compromise, the reviewer suggests to provide enough warning about the reliability of the results and the uncertainty of the method used in both the methodology section and at the beginning of the conclusions. Please consider this suggestion as well as other comments of the reviewer in your next revision.***

RESPONSE:

We appreciate your detailed comments. We have taken care of the concerns of the Reviewer 2 in the responses to his comments and in the revised manuscript.

Specific comments

2. ***Add a "Data availability" section in your paper, which is requested by Climate of the Past***

RESPONSE:

We thank you for bringing it to our attention. This section has been included in the revised version of the manuscript.

3. ***As far as I can see, the astronomical parameters of Berger (1978, Journal of atmospheric Sciences) are used for the 1990, LGM and MIS4 simulations, so this original paper for the astronomical parameters should be cited. Please add Berger (1978) in your reference list***

RESPONSE:

We fully appreciate the reminder. We truly apologise for missing this part in the previous version of the manuscript. We reformulated the caption as follows:

"External forcing used in this study for 1990 CE, LGM and MIS4 conditions. The orbital parameters are calculated according to Berger (1978). Estimates for glacial levels of CO_2, CH_4 and N_2O are obtained for LGM from the PMIP protocol (http://www-lsce.cea.fr/pmip2/) and for MIS4 from ice cores according to Schilt et al. (2010) and Bereiter et al. (2014). Note that the external forcing corresponds to the values of the driving the CCSM4 simulations (Hofer et al., 2012a,b)"

4. ***In Table 1, is "15.22" is typing error? Shouldn't it be 195.22?***

RESPONSE:

We have checked our calculations for the orbital parameters according to Berger (1978) and the value 15.22 is correct. This value was also used in the CCSM4 simulation that functions as initial and boundary conditions. To clarify this, we reformulated the caption, please refer to the previous response.

5. ***Regarding the experiments LGM(FIS50) and LGM(FIS150), the size of FIS is changed only in WRF but not in LGM CCSM4. What would be the impact of this inconsistency of ice sheet setup between the GCM and RCM on the results?***

RESPONSE:

We are aware of a potential effect due to the unchanged LGM ice sheet in the CCSM4 simulation. Nevertheless, we expect only a minor impact of this experimental design on the conclusion since we rather strongly modified the ice-sheet configuration in the WRF simulations. To assure that the reader is aware of a potential impact, we added further information in the section about the model setup (Sect. 2 of the revised manuscript) and in the discussion and conclusions (Sect. 5 of the revised manuscript).

In Sect. 2:

"...Note that this CCSM4 simulation uses 100 % LGM ice-sheet configurations..."

to

"...Note that this CCSM4 simulation uses 100 % LGM ice-sheet configurations for both sensitive simulations...."

In Sect. 5:

"...One potential reason of this weak precipitation response may also be the design of the Fennoscandian sensitivity experiment as the driving GCM has not experienced the changes of the FIS. However, we introduced rather strong ...".

to

"...One potential reason of this weak precipitation response may also be the design of the Fennoscandian sensitivity experiment as the driving GCM has not experienced the changes of the FIS, as the FIS was only modified in the RCM. However, we introduced rather strong ...".

6. ***As land cover could be critical for regional climate, the potential impact of using the LGM(LGM) land cover in the MIS4 experiments should be commented in the paper.***

RESPONSE:

We agree that the land cover plays an important role in realistically representing the climate as demonstrated by the accompanying study of Velasquez et al. (2021) and by other other studies (e.g. Kjellström et al., 2010; Strandberg et al., 2011; Ludwig et al., 2017). We had mentioned this in the introduction and we included additional information about the model setup in the section 2 of the revised version of the manuscript:

"...21 years using MIS4 conditions and using the LGM_{LGM} land cover (described in Velasquez et al., 2021). Note further that the Alpine ice sheet is..."

to

"...21 years using MIS4 conditions and using the LGM_{LGM} land cover (described in Velasquez et al., 2021). We use a LGM_{LGM} land cover because it is the closest approach to a MIS4-like land cover as a gridded European MIS4 land cover has not been developed yet. Even though the LGM_{LGM} land cover might influence the representation of the MIS4 climate, it provides the opportunity to gain insights into the effects of a different orbital and atmospheric forcing, i.e. by comparing LGM_{LGM} to $MIS4_{LGM}$. Additionally, the Alpine ice sheet is..."

Once again, we would like to thank the editor Dr. Qiuzhen Yin. We are looking forward to meeting the editor's expectations.

Best regards,

Patricio Velasquez (on behalf of the author team)

Response to referee #1

We appreciate the time the reviewer invested in reading the manuscript in such a careful and thorough manner. We carefully considered and responded the comments. Please find below our response to each comment.

General comments

1. ***Thanks to the authors for the explanations and answers to the comments. To be honest, I understand the argument on demanding simulations, on the other hand, to spend even a year with RCM simulations is not anything exceptional and I do not think it is correct to neglect some „standards" for dynamical downscaling, established within a long time of its development and used in the activities like CORDEX. Not considering the ensemble experiments, which really need the coordinated effort of many groups (already achieved to some extent in global scale within CMIP6 even for paleo, perhaps for regional-scale the opportunity could be raised within CORDEX – FPS), however, even for single modelling setup, this makes a sense as it can guarantee to some extent the reliability of the study. This is as my opinion the weakest point of presented contribution, especially this is i) the separation to shorter periods of the simulation, which do not allow to develop fully the own RCM climate, ii) very short spin-up, which perhaps can provide quasi-equilibrium in some short time scale in a specific season, but the question is if it is working properly in a full annual cycle, as the processes connected with deep layers are slow and delayed, and iii) the location of domains. Clearly, I understand it is impossible to solve these issues as this means to redo the study, and I do not like to waste the time the authors spend with careful analysis of the results and their interpretation, while I really appreciate the attempt to use this kind of tools in paleoclimate studies. However, I do not think that just a sentence at the end of the conclusions provides enough warning about the reliability of the results. In my opinion, this should be mentioned and elaborated a bit both in the methodology section as well as at the beginning of the conclusions. Significance based on statistical bootstrap technique can provide the information on the significance of differences, although sometimes really rather negligible, but not saying too much about overall robustness of the results, e.g. some results from CORDEX FPS LUCAS show big differences in RCM reaction to land cover changes, which might play a role even in this study.***

RESPONSE:

We thank you for your detailed comments. To provide enough warning about the reliability of the results, we included some information in the section about the method and the conclusion of the revised manuscript as follows:

At the end of the third paragraph of Sect. 2:

"...These 21 and 12 years are further split up into 7 and 4 individual 3-year simulation segments, respectively. Note that we split the simulations to efficiently use the available computer facilities similar to accompanying studies such as Velasquez et al. (2020, 2021), even though regional climate simulations would commonly be

performed in one single simulation. For each segment, a 2-month spin-up is needed in order to allow the land surface to come into quasi-equilibrium. Tests suggest that a 2-month spin-up is sufficient to obtain a quasi-equilibrium of the upper meter of the land surface (Velasquez et al., 2020, 2021).

to

"...These 21 and 12 years are further split up into 7 and 4 individual 3-year simulation segments, respectively. We split the simulations to efficiently use the available computer facilities (similar to accompanying studies such as Velasquez et al., 2020, 2021), even though regional climate simulations would commonly be performed in one single simulation. Note that all segments represent the same climate conditions, i.e. they are driven by the GCM using the same climate state. For each segment, a 2-month spin-up is needed in order to allow the land surface to come into quasi-equilibrium. To determine this spin-up, we analyse each segment and the results show that a 2-month spin-up is sufficient to obtain a quasi-equilibrium of the upper meter of the land surface in this study, i.e. no significant trend in the layers of the WRF land-surface scheme. Accompanying studies also suggest a 2-month spin-up (Velasquez et al., 2020, 2021). Note that the spin-up might vary if the segment starts in another season, which is not possible to test in this study due to the highly expensive model settings...

At the end of the first paragraph of Sect. 3:

"...The bootstrapping technique is applied at each grid point using as elements the annual mean values..."

to

"... The bootstrapping technique is applied at each grid point using as elements the annual mean values. Note that few results may be considered negligible, e.g. due to their very small differences, even though there is a statistical significance given by the bootstrapping technique..."

From fourth line of the third paragraph of Sect. 2:

"...The domains focus on the Alpine region; the outermost domain includes Europe and part of the North Atlantic to capture the influence of the North Atlantic Ocean and the FIS on the European climate (Fig. 1). Furthermore, we us..."

to

"...The domains focus on the Alpine region; the outermost domain includes Europe and part of the North Atlantic to capture the influence of the North Atlantic Ocean and the FIS on the European climate (Fig. 1). Note that the domain setup might influence the Alpine climate, which is not possible to test in this study due to the highly expensive model settings. Furthermore, we us..."

At the end of the first paragraph of conclusion section:

"...Note that the results might depend on the model setup, which is not possible to test in this study due to the highly expensive model settings..."

2. ***Further, concerning the skew-T diagram, I am sorry, but it was a misunderstanding, as documented in the authors' response: Additionally, we have modified the following lines (page 6 lines 185-186):***

"...dew-point temperatures (dashed lines). The latter simply indicate temperatures at which the air becomes saturated and is used to deduce the mixing ratio with height, i.e., the amount of water vapour in the air where the dew point temperature line crosses the mixing ratio line." Which is correct, but was replaced by an incorrect sentence appearing then in other places of results discussion (see specific comments, actually, maybe not all found, please check throughout the paper). Indeed, real actual mixing ratio is given on crossing of dew point curve and saturation mixing ratio line. On the crossing of temperature and saturation mixing line, there is a saturated mixing ratio for a given temperature, which is not saying anything on real actual moisture availability.

RESPONSE:

We changed it in the revised manuscript according to the referee's suggestion.

Specific comments

3. ***Page 7, line 196: I would prefer the use of „saturation adiabatic lines", which is more correct***

RESPONSE:

We changed it in the revised manuscript according to the referee's suggestion.

4. ***Page 7, line 197: Should be probably „air parcel"***

RESPONSE:

We changed it in the revised manuscript according to the referee's suggestion.

5. ***Page 7, lines 202-204: The air temperatures are used to deduce the mixing ratios with height, i.e. the amount of water vapour in the air with height, whose values are obtained from the saturated mixing ratio lines when they are crossed by the temperature vertical profiles. - Actually, the real actual mixing ratios are saturated mixing ratios crossed by dew point lines.***

RESPONSE:

We reformulated these lines in the revised manuscript according to the referee's suggestion as follows:

"...the mixing ratios with height, i.e. the amount of water vapour in the air with height, whose values are obtained from the saturated mixing ratio lines when they are crossed by the temperature vertical profiles. The dew-point temperatures indicate the temperatures at which the air becomes saturated. Both temperatures are used to investigate..."

to

"...the mixing ratios with height. The real mixing ratios with height, i.e. the amount of water vapour, are obtained from the saturated mixing ratio lines when they are crossed by the dew-point temperature vertical profile. The dew-point temperatures indicate the temperatures at which the air becomes saturated. Both air and dew-point temperatures are used to investigate..."

6. ***Page 7, line 221: „ . . . for two glacial states the LGM and MIS4. „ Should be comma or colon in front of the LGM . . . ?***

RESPONSE:

Yes, a colon was added in the revised manuscript.

7. ***Page 8, line 253: The mixing ratios (the saturated mixing ratio lines crossed by the air temperature vertical profile) – Actually, the real actual mixing ratios are saturated mixing ratios crossed by dew point lines.***

RESPONSE:

We reformulated these lines in the revised manuscript according to the referee's suggestion.

8. ***Page 10, line 301: The same as above***

RESPONSE:

We reformulated these lines in the revised manuscript according to the referee's suggestion.

9. ***Page 10, lines 304-305: Hardly to see the difference. Actually, the angle to the adiabats matters and this remains basically the same despite there is slight parallel difference between the temperature curves***

RESPONSE:

We appreciate the referee for bringing this to our attention. Indeed, these lines mislead the interpretation of the figure. Therefore, we reformulated these lines in the revised manuscript as follows:

"...This reduction is more evident in the north-western region (site A), whereas the stability in the central-southern region (site B) is slightly reduced in the mid layer of the troposphere (between 800 and 400 hPa; Fig...."

to

"...This reduction is more evident during winter in both regions, i.e. north-western (site A, Fig. 4a) and central-southern (site B, Fig. 4c..."

10. ***Page 10, line 317: How this is working? How that hard to see a change of wind direction can affect the available moisture via foehn effect, actually, the flow is coming over the Alps anyway.***

RESPONSE:

We agree that this is hard to see in the figure. Since we do not explicitly demonstrate the Foehn process, we weakened the statement and reformulated these lines as a speculation for the dryness in Fig. 3h as follows:

"...the northern part of the Alps, whereas the clockwise turning over the southern face of the Alps in MIS4 leads to reduced moisture availability as the flow dries out when crossing the Alps and reaching the Po valley (Foehn process)..."

to

"...northern part of the Alps. Over the southern face of the Alps, the slightly clockwise turning could lead to reduced moisture availability in MIS4 as the flow would tend to dry out when crossing the Alps and reaching the Po valley (assuming a Foehn process)..."

11. ***Page 11, lines 343-344: Not exactly true for northern part of the Alpine region, I do not see linear response there.***

RESPONSE:

We apologise for this misunderstanding. In winter, the precipitation response is interpreted as linear with respect to the northern hemispheric ice-sheet thickness changes when comparing the difference between $MIS4_{LGM125}$ and $MIS4_{LGM}$ with the one between LGM and PD. We reformulated these lines in the revised manuscript as follows:

"...In winter, the difference pattern in precipitation between $MIS4_{LGM125}$ and $MIS4_{LGM}$ is similar to the one found between LGM and $MIS4_{LGM}$ with overall wetter conditions. Especially, we find significantly high precipitation intensities up to 3 mm day^{-1} on the north western and southern regions of the domain (Fig. 6c). The northern face of the Alps shows a decrease in the precipitation intensities. Thus, we interpret the response of winter precipitation as linear with respect to the northern hemispheric ice-sheet thickness changes. In summer, ..."

to

"...In winter, the difference pattern in precipitation between $MIS4_{LGM125}$ and $MIS4_{LGM}$ is generally similar to the one found between LGM and PD. Especially, we find significantly high precipitation intensities up to 3 mm day^{-1} on the north western and southern regions of the domain (Fig. 6c). The northern face of the Alps shows a decrease in the precipitation intensities. Thus, we interpret that the response of winter precipitation is linear with respect to the northern hemispheric ice-sheet thickness changes. In summer, ..."

12. ***Page 11, lines 359-360: Again, actual mixing ratio is given by crossing of dew point curve and saturated mixing ratio line, actually, changes rather low***

RESPONSE:

We changed it according to referee's suggestion. Additionally, we reformulated this line to avoid any misleading in the revised manuscript as follows:

"...Even though the Skew-T diagram indicates that the relative humidity is rather low, the warmer atmosphere results in a small increase of moisture availability in the middle-to-low atmosphere (water vapour). Note that the higher moisture availability is illustrated by the increase of the values of the mixing ratio, which are obtained from crossing the saturated mixing ratio lines with the vertical profile of the air temperature. This moisture increase is especially true for the central-southern region (site B; Fig. 8c) where there is more precipitable water (PW values at the top of Fig. 8a and c). In summer,...

to

"...Even though the Skew-T diagram indicates that the relative humidity is rather low, the warmer atmosphere results in a small increase of moisture availability in the middle-to-low atmosphere (water vapour) in winter. Note that the higher moisture availability is illustrated by the increase of the values of the mixing ratio, which are obtained from crossing the saturated mixing ratio lines with the vertical profile of the dew-point temperature. This slight moisture increase is found in the central-southern region (site B, Fig. 8c) where there is more precipitable water (PW values at the top of Fig. 8c). In summer,..."

13. ***Page 12, lines 364-365: Again, the actual mixing ratio is given by dew point line crossing with saturated mixing ratio***

RESPONSE:

We changed it in the revised manuscript according to the referee's suggestion.

We would like to thank the referee for the time invested in reviewing our manuscript so carefully. We look forward to meeting the referee's expectations.

Best regards,

Patricio Velasquez (on behalf of the author team)

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